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I AM anxious to bring before the Society, as succinctly as possible, some observations on the influence of the discharges and nervous shock on the collapse of cholera. I do so feeling that the proper discussion of the subject in a Society like the present is best calculated to develop what is theoretically valuable in Pathology—what really serviceable in treatment.

The disease, it is true, sleeps for the moment but, if we look to the history of former epidemics and watch the local rekindlings of the last, there is reason to fear that the cause may be still latent, possibly to awaken to its accustomed activity.

During the pause, therefore, we ought calmly to survey our past doings and if we have been found to cling so fondly to any “idols of the den” as to give our theories too much rein, we shall find wholesome restraint in the greater wisdom and moderation of those around, so that good must accrue both to the individual and to science at large.

Unity of opinion on a subject so intricate and one, still hot with debate, I am well aware, cannot be expected, nevertheless, a very cursory survey of the actual position of the pathology and therapeutics of cholera must convince us that a much wider divergence exists than is compatible with the safety of the community and the intelligence of the profession.

It would seem the veriest platitude formally to affirm

that, if the theory were unsound the practice probably would be unsafe yet, viewed in its practical working the proposition cannot be overestimated, since life is the stake we play for.

A divergence of opinion then so manifest does not speak hopefully for our science. Indeed, despite the hopes for years indulged, the old question as constantly asked and as variously answered since 1832, still presses on us under the additional light it is fancied that we have for a final reply.—Is cholera the result of a specific poison inhaled into the lung and passed into the blood, working mischief there, yet with a tendency to arrest by a salutary outpouring from the stomach and bowels? Such outpouring being thus secondary and, possibly, conservative ; or, is this outpouring an essential part of the disease—equally the result of a poison but located in the gastro-intestinal tract and reflecting its violence over the system?—the outpouring, thus, being primary and certainly destructive.

Such is the dilemma to which I specially invite your attention.

Permit me, first, a brief allusion to one, not only of the most controverted but, of the most interesting points in the morbid anatomy of cholera from its bearing upon the progress of the disease viz. the condition of the great organs of circulation and respiration. It is allowed that in the various epidemics, both symptoms and *post-mortem* results somewhat widely differ. Still, after very extended and, I hope, impartial investigation of the subject, to my mind, the mass of evidence in favour of stasis in or, the transit of dark blood, through the lungs, is so overwhelming, that it must be considered as a more or less general fact in the algide stage of cholera. To doubt it, would be to impugn either the good faith or competence of the numerous observers. In earlier times, without any special theory to support, we may not, for one moment, challenge the one, and the simple evidence of educated sense is, surely, a sufficient guarantee for the existence of the other. The test of the balance has, in some cases, shown, as might be

inferred, the congested lung to be heavier than normal. In a few instances, it is stated that the left ventricle was alone distended with black blood. I have, elsewhere, attempted to show that changes in the distribution of the blood may occur *after* death but, that these latter phenomena are not so explained, will be subsequently seen. On the other hand it must be confessed, however we explain the matter, that very many examinations have shown the blood to be arrested at the right heart, and the lungs and left heart, left, comparatively, bloodless. This condition has been particularly adverted to by authorities at home and abroad. At home, by Dr. Parkes whose researches are so often and so deservedly quoted. In his summary, the only point we miss in this portion of morbid anatomy, is the condition of the coronary arteries; such condition must have a significance for, if arrest of blood occurred at the right heart, these vessels would bear to the cardiac structure blood, deficient in quantity,—if black blood passed, defective in quality; on either alternative, the normal balance of the heart would be disturbed. Furthermore, the state of these vessels might furnish a key to that congestion of the coronary veins Dr. Parkes so frequently observed and, which, else, would stand out as a bare fact, recorded, but unexplained. If little or no blood entered the left heart, the explanation of such distension of the vein would not always appear upon the surface of the matter, seeing that the blood of the turgid right auricle could not force the coronary valve. If, on the other hand, impure blood of given quantity entered the nutrient arteries of the heart, we might account for the distension of the veins and the dark ecchymoses often surrounding them by the *stasis* induced by such blood in the cardiac capillaries, and veins. Simple *plenum* of the right auricle would not prevent the ingress of the coronary venous blood, since its walls are often flaccid. Apart from this, however, none of the results described are peculiar to cholera, they equally occur in algide fever, yellow fever, serpent and other poisons and, probably, in other diseases also. So that, in epidemics of cholera, if

we look at all that *post-mortem* examinations unfold—appearances sometimes slight, sometimes variable, at other times, opposed, and, often, common to other diseases—we must concede that mere morbid anatomy fails to satisfy our wants as to a pathology of cholera.

To proceed to our more immediate subject. The first point for consideration is as to the existing amount of evidence for the presence, in the blood, of some specific poison. I commence with one of the weakest points that could be urged on any ground of opposition to such view, indeed, I state the facts here rather for the purpose of obtaining your opinion than for that of wielding them against the hypothesis in question. I refer to the bursting out of the disease in the midst of perfect health, and will simply take two illustrations from Indian experience. In one case, an officer had been out tiger hunting in the morning and returned to quarters in the highest health and spirits; during breakfast, he was suddenly seized with watery vomiting and purging, and the disease was, almost immediately, confirmed. In the second case, another officer in the very plenitude of health and spirits, went, after breakfast, to bathe with his companions; while in the water, serous vomiting and purging instantaneously set in and collapse speedily followed. Do either of these cases find, in your minds, satisfactory explanation on any theory of a poison in the blood? can you imagine a disorder so produced attaining its meridian of violence suddenly in the midst of vigorous health and elastic spirits or, a discharge to be critical without our having the slightest insight into the necessity for a crisis; and surely to avoid an inconsequence, if we affirm “critical discharge” we ought to have some knowledge of the state related to this predicate, otherwise we oppose a positive term to the vaguest possible correlative. Besides, what could a critical discharge super-add to perfect health? At least, whatever doubts you may entertain, are not the symptoms quite as reconcilable on the supposition of some change originating in the gastrointestinal tract and called into action by the stimulus of

the food in the one case and by the sudden revulsion, in the other, on the mucous membrane?

Next, if cholera poison can, by inhalation, pass into the blood, it ought equally to be capable of exhalation and, thus, of reproduction. A large experience opposes itself to this hypothesis—practitioners, nurses, friends are constantly breathing the very breath of the sufferer, examining the fresh discharges—and when does a clear case of propagation thus occur? That attendants, occasionally, are attacked by cholera is very true, but, comparatively, so rarely, that when an epidemic influence is present we may as fairly assume such attack to be, possibly, a simple coincidence.

I am far from denying the contagiousness of cholera under certain conditions, doubting, only, whether it be through the medium of *recent* excreta.

The blood of cholera patients has, in many cases, been passed into the circulation of healthy persons, from accidental wounds or intentional inoculation, and, never, with any untoward result. The blood, in larger quantities, has been injected into the veins of animals without producing true cholera; ill results have unquestionably followed, but these have been nearly conclusively shewn to be the result of blood *sepsis*, and not choleraic.

Infants, again, have often been suckled by mothers suffering under confirmed cholera but, in no instance that I am aware of, has the disease been communicated to the child thus exclusively nourished, prior to the actual invasion. A secretion so prone to be influenced by matters in the blood, and to influence the offspring seems, here, to bear no poisoned taint. It must not, however, be forgotten that cholera is rare in infants at the breast, but this fact does not at all explain the immunity, if the blood circulate so subtle a poison as that of cholera.

The recent excretions, just separated from the blood, and which ought, equally, to be most infectious, have been repeatedly tasted and even drunk by man, and injected into the veins, stomach, and bowels of animals, without sensi-

ble influence over the health. In fact, the former series of experiments has been repeated so many times upon the human subject without injury, that I attach very little importance to experiments on animals where results are said to have been produced. The researches of Pettenkofer and the latest experiments in Paris, tending strongly to support them, show that the *recent* excreta are innocuous when introduced into the system ; some short time must elapse until decomposition furnishes the *materies morbi*.

If we further look at a certain proportion of cases in which sudden relapse occurs, 14 or 16 days after apparent convalescence and nearly always resulting either from error in point of quantity or, quality of food or, from the operation of cold and damp, we cannot reconcile, with our present knowledge, such relapse with the notion of a poison in the blood. To suppose that such an active agent could, thus, rest dormant, would amount to a virtual denial of its leading attribute, and seems at variance with any theory of elimination.

The researches of Dr. Beale, showing alteration of the intestinal villi, apparently of some duration, do not support the theory of an eliminative poison of the blood nearly as much as that of some agent acting from the intestinal tract.

Next, with regard to the theory of elimination. The term, though in current use, is not at all times very strictly defined as to its extent of application. We, surely, ought severely to distinguish between simple and critical elimination ; they clearly mean very different things. With reference to the pathology of cholera especially is it more than ever necessary to define our terms. We know that certain remedial agents may exhibit special affinities for certain organs ; thus, juniper may tend to the kidneys, squills to the mucous membrane of the lungs, elaterium to the stomach and bowels, mercury to the salivary glands and so forth. These agents may, though the fact is not proved, eliminate themselves through these several tracts with the secretion they increase, and, even carry with them

injurious matters from the blood ; though the latter result would, probably, in a logical sense, be an accident of their action. Few such agents have any special power to select and carry off an actual poison from the blood ; they eliminate, and, in the current, sweep away morbid matters. The agent employed is obedient to certain laws. From a multiplied experience we anticipate the effect and can control it often at our will. Should the agents act with undue violence, they require restraint. . . But we should hardly venture to apply the term *critical* to such discharges. A critical elimination bears the impress of something like intelligent design on the part of Nature, and, on her part alone. We can rarely predicate either the time, manner, or even site of its occurrence—now, by hæmorrhage—now, by abscess—at one time, by a sweat, at another time, by a diarrhœa, sedimentary discharge from the kidneys, or, by long and restorative sleep. Whichever of the selected modes, they all possess, in common, the characteristic of affording speedy and direct relief to the maladies in which they occur, and this generally happens during the stage of reaction. This is precisely what we should expect from the extrusion of a poison which had vitiated the composition of the blood and aroused a reactive energy of the system and should hesitate to apply the term *critical* where no corresponding relief occurred. This is the rule. But, even in cases where Nature oversteps her mark and such discharges exceed the limits of relief, no sound practitioner ever hesitates in an attempt to moderate their violence.

In *confirmed* cholera, all is reversed. Gastro-intestinal discharge is the constant, overt, *initial* phenomenon ; it occurs under depression, and progresses and culminates under exhaustion and collapse. Even assuming, for a moment, cholera to result from a poison in the blood, if we question ourselves, how little do we know of any such agents ! Absolutely nothing as to their essence, and but little more as to their proximate action. We are only cognisant of the wreck they occasion.

But, if we were, provisionally, to assume that a volatile

matter entered the blood in some definite quantity or, to form with some element therein, a definite concrete (the hypothetical cholera-cathartine) the nature of the disease would be intelligible enough, we might reasonably infer that such a compound from its drastic essence, might eliminate itself and bring its own cure or, be restrained by treatment.

So conceived, a theory of simple elimination would be rational and in harmony with what we know of drastics acting by absorption, and, in the category of these, it might be included.

But, if the poison be regarded as zymotic, fermenting the total mass of blood, then, I apprehend, such an action would furnish the best argument for our assuming that cholera poison contributes to a fatal issue by drain of nearly all the available serum of the body, and, *a fortiori* a sufficient reason for regarding the discharge as non-critical and for checking it if we possessed the power.

In an avowed ignorance, then, of the exact operation of this blood poison, it is open to all to speculate and guess, for guessers are somewhat like archers, now and then, an outsider gets a gold centre. I would make one trial which you will accept *quantum valeat*. If we are to assume that cholera really be an inhaled poison, and to believe, as I fancy we must, that it is not directly infectious, I would ask whether some volatile element inhaled, might not form with another element of the blood a definite concrete—a dangerously depressant emeto-cathartic, which thus acts only as a poison to the individual. When the excretions are passed, after two or three days, decomposition occurs, the volatile agent is again set free, and either held in solution, or volatilizes and passes away to react upon some other *predisposed organism*.

Such, is the doctrine of a poison in the blood, and, of the contingent effort for its removal by discharges from the stomach and bowels ; a doctrine most seductive, and possibly, sound, though, to some minds, wanting in the full evidence to make it binding on belief. We may, there-

fore, very properly seek to obtain either a reversal of the sequence or, at least, a suspension of opinion, should equal or better evidence be adduced in favour of a different view.

We might submit that the internal or external drain and coetaneous shock upon the nervous system is the essential—is the actual disease, without which, it would not be cholera; and, that true algide phenomena do not occur either before or, without, the drain or, whatever else, we term that matter either fluid, or semi-solid which fills the stomach and bowels, consisting of the spoiled elements of the blood. In nearly every language, cholera implies a kind of colic, flux, or draining from the bowels with rapid loss of power. This is the one great result which remains permanent under all the varying conditions that post-mortem examination reveals.

The history of the various epidemics clearly shows that the disease may be ushered in by *prodromata* or, that these may be entirely wanting.

Where such exist, as, for example, lassitude, vertigo, singing in the ears, disturbed vision, pallor, syncopal tendency, anorexia, thoracic, or abdominal uneasiness; they are quite explicable on the supposition of some local disturbance of the gastro-intestinal tract—irradiations from the great epigastric centre and intestinal plexuses; indeed, several or nearly all of these symptoms attend, to some extent, the commonest disturbance of this tract, so that no necessity need exist to push back this semeiology to prior changes in the blood. We must not forget one first great rule of philosophy “*entia non multiplicanda sunt, præter necessitatem.*”

In other cases, diarrhœa, variable in duration, at first without compromise of the general health, and nowisedistinguishable from a common looseness save by its endowment, potentially, to spread the disease, precedes and, in an immense majority of cases, gradually or suddenly merges into confirmed cholera. On this point, the opinions of the most experienced men, of all countries, are almost unanimous.

If this diarrhœa is checked which is generally practicable in the earliest stage, health is, at once, re-established. This unquestionable result again, strongly pleads in favour of the local origin of the flux, and stands in striking antithesis to the doctrine of a poison in the blood requiring elimination by such an outlet, and which being hemmed in, ought, *ex hypothesi*, to be followed by alarming internal disorder, since Nature never permits us to arrest a truly critical discharge with impunity.

When the disease suddenly explodes as confirmed cholera, as it does at times, and in the midst of seeming health, the site again is local, whatever the cause may be. A double change almost simultaneously occurs—shock upon the nervous ganglia (in some very rare cases so intense, as rapidly to arrest the higher functions of respiration and circulation, and kill before more marked phenomena are seen;) and sudden rush of serum of the blood through the walls of the stomach and bowels;—a rush, so enormous that as Moreau de Jonnès describes “it seems, at times, greater than the entire mass of fluids in the body” or, to quote another great authority, Cruveilhier, “this enormous abstraction of the serum of the blood of the cholera patient in a few hours, nay even in a few instants, explains the symptoms of the malady much better than any alterations of structure.”

This sudden and immense drain of blood elements may, in those rapidly fatal cases just referred to, simply fill, to distention, the stomach and bowels, without outward issue, thus resembling internal hæmorrhage, though hidden from sight, still, lost to the circulation; revealing itself to our sense by palpation and percussion; the features expressing the loss the circulation has sustained.

Here, you will pardon me a short digression with reference to the subject of transudation through the bowels. It has been of late, too much the fashion, if not to ignore, at least, to under-estimate this influence in the causation of collapse. According to my experience, a graver oversight could not occur in the pathology of cholera.

Those pathologists, of course, who look upon collapse under the light of another causation, and regard such condition either as resulting from the special action of a poison on given vessels or, from the shock communicated to the nervous centres from an inflamed condition of the bowels, would regard the discharges as of subordinate importance. With every respect for those who hold such views, and held, possibly, on a securer tenure than my own, I still must say that, to disregard the influence of discharge appears to me, a backward step, in every sense in the pathology of cholera. Such is its significance that I think it fair to call your attention to some facts connected with the influence of intestinal discharge upon the circulation in its least complicating shape, we may, then perhaps, better estimate its influence in cholera.

Many have read the narrative of Morgagni. This great physician, once travelling post, to visit a patient, was seized with sudden looseness of the bowels and passed, successively, in twelve hours, sixteen pints of serous fluid. In this brief interval, he had become as emaciated as if he had suffered from some long and wasting malady, and his weakness was extreme for several days.

Cruveilhier states, that when at Limoges in 1818, during a very hot summer, he was called to a woman 40 years of age whose features were so profoundly changed, her face so sunken and discoloured, that he supposed the case one of abdominal disease in the last stage. Have you been ill long? "No!" was the reply in a hoarse voice. "This morning I left my house on business in perfect health. On my return, I was seized with a looseness and passed, rapidly, an enormous quantity of water." So that in a few instants this woman was perfectly prostrate. The same physician, again, states. "I was sent for at Paris in March 1849 to a young girl from Auvergne who retired to rest in her usual health, and whom I found dying at 7 A.M. Upon entering her small chamber, I saw the floor inundated with water. It was cholera discharge passed, involuntarily, during the night and which had

soaked through the mattress. She died, a few moments, after my arrival."

To avoid being tedious, I will only cite one more example, quoted by Trousseau and Pidoux, from Barbiers work on *Materia Medica*, and which he personally observed—A woman, in one of the faubourgs of Amiens, took a purging medicine from a herbalist, it caused such excessive discharges by vomiting and purging, that rapid exhaustion ensued. Being conveyed to the Hôtel Dieu, the following morning, the tip of the nose, the ears, and cheeks were of a very dark violet colour as, also, the hands and feet. Gangrene rapidly followed, and she lost one of her feet, and some phalanges of the toes of the other.

Now, we must remember, with one exception, these were the results of simple drain in healthy constitutions, and had they lasted, would not the truest algide collapse speedily have followed? They closely resembled, in their effect upon the system, those of cholera stopping short of collapse. The occurrence of gangrene has been several times observed in cholera, and doubtless from the same cause.

Dr. Leney, a physician practising in Bray, has, within the last week or two, recorded cases of what he terms "*Contagious Hamorrhæa Serosa*," a disease it appears, differing from epidemic cholera; here, the discharges were immense, cessation of the pulse, and other phenomena of collapse rapidly occurring.

An escape of the constituents of the blood so sudden, rapid, and excessive, seems almost incredible until we reflect upon the vast extent of the surfaces engaged, the capacity of the tubing to receive and, for a time, the almost boundless sources of supply. Secretion it is not, but one almost incessant current from myriads of minute converging streams knowing no let or hindrance save that afforded by the frail barrier of the limiting membrane of the tract.

Researches, comparatively recent, have thrown some light upon the physical phenomena of currents traversing membranes, and the means to moderate or, even to reverse, their action.

Dr. Alderson, in a recent lecture on cholera, has ably applied the experiments of *Matteucci* and *Bacchati* respecting the process of exosmosis, to the escape of fluids in cholera. There can be no doubt that his explanation greatly aids us in understanding the physical phenomena of the current, but it fails (as I suppose he would concede,) when applied to the higher causation of the malady; it does not reach those vital changes which precede and determine the rush of fluid to the membranous walls of the stomach and bowels.

Not to break the filiation of the argument as regards these simple, though profuse evacuations, we have to consider them in their immediate connection with cholera—as the destructive and not eliminative elements of the disease. If we bear in mind the fluids lost by the premonitory diarrhoea, and the amount of serous vomiting and purging coinciding with and following the developed disease, such a dissipation of the fluids seems alone sufficient to produce the extremest degree of collapse independently even of the primary nervous shock to be referred to subsequently. I reproduce the testimony of Schütz of Berlin who has had immense experience in cholera. He states, I have known patients brought into the hospital with but moderate vomiting and purging and no unusual depression of temperature or of pulse; before my very eyes, these patients suddenly passed *enormous serous discharges from the stomach and bowels*, and *pari passu*, sunk into the most perfect algide stage. This is no unusual result, cholera literature abounds with such. I do not see how such losses can be regarded otherwise than destructive, totally disproportioned to any critical requirements, and, determining all the symptoms it is our object to prevent.

So far for the *outward* indications of discharge. Let us now trace it in its *hidden* form. This stage of the pathology of cholera is of the highest importance to investigate, since it is alleged that cholera can prove fatal, by sudden collapse from the influence of the poison on the blood, without the customary drain. The power of cholera, suddenly

to kill, I can neither affirm nor deny, but, I may say that such a mode of death is not seen in this country. That an intense influence of the agent when it has begun its action in the gastro-intestinal tract may quickly, indeed, simultaneously, paralyse the heart, intestines, and, arrest nutrition, is certain, though never, I believe, without internal transudation. The fact, however, of comparatively sudden death has been so confidently stated, that we cannot evade such fact, neither ought we to attempt it. Mr. Jameson, in his graphic account of an outbreak of cholera in the camp of the Marquis of Hastings and, subsequently, during a march, speaks of hundreds rapidly falling, dying or dead. We must, however, liken this to the general description of a battle ; striking incidents, only, are narrated, while minuter detail is left to later despatches. But, with regard to this very class of cases, the experienced Indian pathologists are scantiest in their *post-mortem* results. In any disease, with a well known pathology, we may, occasionally, find exceptions in some particulars, which would not vitiate the general result ; but when such exceptions are selected to form the *basis* of a theory, it clearly behoves us to challenge and to scrutinize them most exhaustively. I must, therefore, endeavour to supply the *post-mortem* deficiency alluded to, from the experience of investigators nearer home. Cruveilhier, whose reputation as a morbid anatomist needs no eulogy of mine, made between two and three hundred examinations of cholera patients, and thus expresses himself with reference to cases unattended by much outward discharge. "It was particularly in individuals who had only passed a few choleraic discharges that this enormous quantity of fluid was discovered in the stomach and bowels after death;" again, "some patients did not pass during life, one single alvine evacuation, but, notwithstanding, their intestines were found after death, filled with the peculiar fluid." Those most accurate pathologists, *Reinhardt* and *Leubuscher*, state "In general, death occurred after the period of the very frequent and excessive discharges ; there were exceptions, however, for many

patients died before the rice-water secretion contained in the bowels could be evacuated." Scot, also, declares "that the stomach and bowels have, sometimes, been found after death, filled with fluid, though no vomiting and purging had been observed." To finish with one more very recent statement; Dr. Leney, whose observations have been before referred to, in drawing a contrast between "*Hæmorrhœa Serosa*" and epidemic cholera states, in reference to the late epidemic of the latter in Ireland, that those cases in which the secretions were retained, no evacuations taking place from the stomach and bowels, proved the most suddenly fatal, and, on post-mortem examination, both stomach and bowels were found distended with sero-albuminous fluid, almost to bursting. Numerous other confirmations might be cited to show that, while some patients die from shock and profuse external discharges, others, die from shock and from the same discharge though, during life, it is concealed from view, and not from any poison uneliminated from the blood. We must not, therefore, when a patient is brought into hospital, cold and pulseless, yet with but little drain, infer they die from poison in the blood; their history generally shows profuse antecedent discharges, and the abdomen is still dull to percussion and resistant on palpation; neither must we forget that with the squalid and ill fed poor, though no real disease exist, their power of vital resistance is at times so low that discharges, comparatively small, may cause a fatal collapse. That recovery can take place under drain, is certain, but not as it appears to me, from any further elimination of poison from the blood. The excretion of fluid, is, I am quite sure, sometimes the result of returning power of the canal, and also, an evidence of the return of general reaction.

From the consideration of this amount of serous fluid either ejected from or retained within the gastro-intestinal canal, we naturally turn to the condition of the blood from which the abstraction has been so suddenly and directly made. How greatly changed it is both in its vital and physico-

chemical endowments, all admit, however opposite the theory that each may hold. We may even proceed a step further and affirm such changes to be sequential, at least, that none, appreciable to sense, attend the onset of the disease. *Majendie*, than whom none could have had a larger experience in cholera, inasmuch as he attended between five and six hundred cases, states "I have seen on numerous occasions, in England, bleedings before the algide stage, at two or three hours interval; it was not possible to detect any difference between the blood drawn and that of a non-choleraic subject. * * * It is worthy of remark that the blood is not changed at the onset of the disease, but gradually, as it progresses, hence I infer that the alteration is the consequence and not the cause of the disease. Permit me further to quote the testimony of two very competent Indian observers. *Seot* writes "It is established by the replies to this letter, as well as by an immense mass of concurrent evidence, that the blood is of an unnaturally dark colour and thick consistence. These appearances are very uniformly expressed by the terms dark, black, tarry, in regard to colour; and by thick, soapy, syrupy, semi-coagulated in respect to its consistence. The change in the condition of the blood is, likewise, fully proved to be in the ratio of the duration of the disease; the blood at the commencement seeming to be quite natural, and, more or less rapidly assuming a morbid state as the disease advances." *Searle* also, states "It is established by undoubted evidence, that the blood is of an unnaturally dark colour and thick consistence; these changes in the condition of the blood are, likewise, fully proved to be in the ratio with the duration of the disease."

The examination of the residual blood shows, that, in all epidemics, the total amount is diminished. In some cases, doubtless conditioned on the extent and continuance of the drain, the quantity remaining is so small, that scarcely a pint can be collected free. This amount of varying consistence, is massed in the heart, lungs, pulmonary arteries and veins. The systemic arteries are generally empty,

and the large organs more or less anæmic. From the admitted density of the residual blood, and from the special losses it has sustained, we can easily conceive many of the physical characters accorded to it. But, with regard to the changed colour of the blood? not a relative darkness from simple aggregation, but one absolute, and all important. Let us not forget that the black colour did not exist at first; whatever its cause, it is the consequence of the *progress* of the disease. It cannot always be the result of obstructed passage through the lungs, since, during the early part of the cyanotic stage, the left ventricle contracts, and arterial pulses are still, though feebly, to be felt. They do not beat on air, and we can have no better proof they circulate black blood, than the fact well and repeatedly ascertained that, if an artery is opened during life or rather before its immediate close, the blood exhibits the same well-marked black colour. With a view to the possible solution of this problem, we must enquire afresh into the effect of the sudden and excessive abstraction of the plasma as specially affecting the blood disks. When a given quantity of the plasma has left the main vessels, and all available sources are cut off for remanufacture, the disks are said by *Schmidt*—one of the very highest authorities on the subject—to permit exosmosis of their proper nutritive elements into the main current. They yield in part their globulin and hæmatin—elements as important in the life-history of these microcosms, as they themselves are in that of the macrocosm of the body. So that we have injured disks and white corpuscles retained in a fluid which has been robbed of its water and salts, partly spoiled as to its fibrine and albumen, and altered, as to its consistence. It is precisely at this juncture, as I believe, that change of colour commences which explains, in part, the subsequent phenomena. If the nutrition of these minute, though important organisms is dependent upon a standard composition of the blood plasma, and, if this fluid is modified as supposed, is it surprising that the disks, after the loss of their own in-

ternal pabulum, should participate in the disorder and become, at least temporarily paralyzed as it were, and unfitted for the object of their ceaseless journeyings—outward bound with oxygen for distant capillaries, and homeward laden with carbonic acid which they should discharge into the air cells. That they are, now, inadequate to the final end of their normal freighting is probable, since they pass the lungs unpurified and onwards to the left heart, systemic vessels and coronary arteries, in which last, they powerfully concur, in combination with nervous shock to bring the circulation, gradually to a stand still. By nervous shock is implied, not alone the primary depressing influence of the poison, but also, the exhaustion induced by vomiting, purging, and cramps.

There is nothing speculative in the above statement, for *Rayer* has conclusively shown that no chemical change occurs during either inspiration or expiration as the algide stage progresses ; hence, the warm breath of health is replaced by one of unnatural coldness.

With regard to respiration generally, the respiratory centre is but slightly modified, the mechanical part of the process is essentially perfect, though, often, accidentally impaired by the severe cramps of the abdominal, thoracic, and diaphragmatic muscles. In a published memoir, I have stated as the result of my observations, that embarrassed breathing is not of constant occurrence in cholera. So essential is it to have a clear understanding on this cardinal point that I shall quote fuller evidence in support of my assertion. “Respiration is often nearly natural when the pulse is gone.”—*Mackintosh*. “In the algide stage the respiration is sometimes natural so far as regards the number of respirations and the extent of the movement.”—*Majendie*. “In the algide stage, the respiration is sometimes natural, at others, quickened or slackened.”—*Cruveilhier*. “Respiration is not usually interrupted in the early stage unless by spasm.”—*Scot*. “It has been shown that *cholera asphyxia* affords a remarkable illustration of the fact that respiration may continue free after the circulation,

to all appearance, has ceased.”—*Bell*. “Respiration is not, usually, interrupted in the early stage of cholera. In many cases terminating in death, respiration has gone on in its mechanical part with little or no interruption except, that it has become slower and slower. . . . In natives, respiration is, generally, pretty free until the very last.”—*Searle*. “In many cases the respiration was quite calm till death.”—*Reinhardt* and *Leubuscher*. “I have known the respiration to be quite easy, and noted it as such, while the cramps, coldness, small, rapid arterial pulse and inelastic skin indicated the advance of collapse.”—*Horace Jeaffreson*.

It appears, then, that in a very large proportion of cases, no true lung dyspnoea—no marked “*besoin de respirer*” necessarily exists although in some cases, it has, doubtless, been observed.

The testimony afforded is that of the very best observers,—and the result of pure clinical study.

Every theory of cholera must, in common with the other established *data*, submit to be tested by such evidence, and, if it will not stand such test—will not square with all the pathological facts of the disease, it fails in the required comprehensiveness.

During the cyanotic and glacial stages we may infer then, that some amount of black blood circulates for a time, as in cases of congenital cyanosis, but why, in cholera, the brain, the only support left of the great tripod of life, should preserve almost to the last its higher functions intact, is a problem which neither the time nor my ability would permit me, properly to discuss.

That the condition of the blood is such as has been described is rendered more than probable from the effects so instantaneously following saline injections into the veins. Shortly after the fluid has entered the vessels, the colour improves, the temperature rises, the pulse returns, and the tissues again fill out, showing that hæmatisis is restored. When the injected fluid transudes, the collapse stage again returns, to be again removed by fresh supply, so that,

cæteris paribus, this alternation might be almost indefinitely prolonged. In some well nigh hopeless cases, as in the recent remarkable one recorded by *Lorain*, one injection of 400 grammes of water at 40° Centigrade, saved the patient's life. Such results would seem to show that the condition of the blood does not so much depend upon any active general poisoning as upon such an abstraction of the nutritive material from the globules and plasma as temporarily arrests the physico-chemical conditions under which they normally act.

Algide fever, of all other affections, best illustrates the second influence contended for,—*shock*, or whatever neurotic change is comprised under it.

This remarkable affection, one of the forms of that protean malady, "*Pernicious Ague*," has been so thoroughly investigated by *Maillot* that we may confidently trust to his description.

The phenomena of alidity appear, not as a termination of the ordinary cold stage, but often, amid the best marked reaction. Suddenly, the pulse becomes quick and weak, falters, then ceases altogether; now,—but always subsequent to the arrest of circulation, the temperature, though unfelt by the patient, rapidly falls to a glacial point, and the surface assumes the cyanotic hue; respiration continues free; the cerebral functions are intact to the last. Some instantaneous change has unmistakeably occurred in the nervous centres, no other view could account for such phenomena. Life is at once in the balance, and should serous vomiting and purging supervene, is almost certainly lost.

Here, the analogy between the two affections appears most striking and strongly points to the neurotic character of both. Pernicious ague might explain the death from coma said to occur in cholera, coma being one of the most common terminations of the former disease; it might further aid our explanation of those cases characterized by such rapid failure of the circulation and the temperature as are described in cholera: in both, the blood is black and stringy; in both, the heart is robbed of its contractile power.

Cholera, by universal consent, has been regarded as a mysterious disease. It doubtless is so. Yet more, perhaps, in the sense of the riddle of its proximate causation than in that of the fulness of its pathology. It is not like a violent shock fatally depressing the circulation through the nervous system; here, there may be no loss. It is not like a simple, though dangerous hæmorrhage, since here, although the vessels pour out their contents yet, should the circulation rally, there still is left some pure blood, a contractile heart, and magazines operative for repair and immediate fluid supply to the vessels. It is not, altogether, like pernicious ague, for here, paroxysmal recurrence of algide phenomena is the law. Cholera is, so to speak, the sum of these influences combined. It is the result of some special agent profoundly modifying the function of the extensive gastrointestinal tract—rapidly radiating back a paralyzing influence on the great nervous ganglia connected with the spino-sympathetic centres, and suddenly abstracting from the circulation, under such depression, a large amount of blood constituents without any capital left wherefrom to write off the loss. The vital powers thus taken, as it were, by surprise, too frequently yield before they can, in any way recover.

Cholera, seems, then, in its very essence a disorder of function and, moreover, one of special functions. Upon reviewing the phenomena of animal and organic life, we found in the former, the intellect intact, volition to some extent, still active, and the muscles obedient to its mandates; the volitional play of respiration unless from spasm, unimpaired. Look to the spino-organic system and all the disturbance that we find, seems centered here, so that if it were not so old-fashioned, one would still maintain this system to be the great centre of *functional*, I do not say *organic* change, for that would bring back the old objection that no such changes are patent to our coarser or even aided vision. But, if cholera be a functional perversion, what changes could we expect to find?

Numerous theories have been since advanced, many of in-

terest enough to arouse attention ; none of weight enough to enforce belief. The old view, then, associated with the names of *Loder* and *Delpech* may relift its head, though robed in modern garb—in a functional and not organic aspect—as a secondary, and not a primary causation.

To ascend to this causation, two conditions must be satisfied ; adequate knowledge of the laws which govern the acting agent, and a more precise acquaintance with the functions of those portions of the nervous structures upon which such agent does directly or, indirectly act. The latter knowledge may, probably, be a future conquest of Experimental Physiology. We want *Claude Bernard* to do for the abdominal, that which he has done for the cervical portion of the sympathetic. But, if we could admit *paralysis*, equivalent to *section*, we might even carry the light of his researches here, and find a given sameness in result, decided augment of temperature of the internal parts of the abdomen and, an undue supply of blood to the capillaries of the gastro-intestinal tract.

I have thus adopted what appeared to me the easiest method of accomplishing my task. If the nature of the disease is sought for in its fully developed, complex state, all past experience shows that it eludes our grasp. But, if the method is reversed, and we estimate aright the simpler elements—what part the discharges play, and what the nervous influence, the difficulty of the enquiry may be thereby, somewhat lessened.

In conclusion let me observe that this paper is to be regarded simply as suggestive. The time, seemingly, has not yet come to justify any dogmatic assertions. We must still pass the old question on. We require, it is superfluous to say, a constant faithfulness in our record of individual cases—a pure record unvarnished by any theory ; but what I think a still more urgent need is, of a good clinical and pathological history of the various epidemics, showing to what extent, in each, the disease may have departed at times, from its ordinary type ; and tracing, if possible, the conditions under which such varia-

tions have occurred. This is the only way to reconcile differences which, unquestionably occur, and to explain contradictions which are awkward and illustrations of the common fallacy of drawing general pathological conclusions from the *data* of a particular epidemic.

I have endeavoured, to the best of my ability, with one exception, to keep as evenly in the track of fair argument and established fact as the subject would permit. To ascertain to what extent you can approve the argument and accept the facts, is the object of the paper you have been good enough to listen to.

